persuading the authorities that they will put the building out of harmony with the other structures on the campus. Hence the chemist must himself tackle the problem in detail. Then again, if the laboratory operations occupy long periods of time, the intervals between the points at which thought by the student is required, or the practise of certain manipulations is demanded, are so prolonged that the pupil forgets to think when the time comes, and bungles the manipulation because his mind has long since wandered to some other subject. Thought and physical activity are more effective when there is a more or less continuous demand for them, and so every abbreviation of the periods of waiting and of the interruptions, caused by looking for some article or going to a hood, increases the efficiency of the work as a form of study. It also, of course, permits more work to be done, and therefore more subjects for thought and more manipulation to be introduced, and so gives more mental training and greater technical skill.

The magnificent addition to this laboratory, the opening of which we are now celebrating, has been made at a most opportune time. A German statistician has discovered that the ratio of chemists to population in four countries is represented by the numbers: Switzerland 300, Germany 250, France 7, Great Britain 6. The corresponding number for the United States is probably nearer to the two last numbers than to the number for Switzerland. The general run of people in this country, even educated and intelligent people, have hitherto been almost entirely unaware of the important rôle which chemistry plays in the industries. When you tell them that many railroads employ fifteen or twenty chemists each, they stare in astonishment, and can not imagine what there is for a chemist to do in such a connection. But the discussion raised by the war has suddenly drawn chemistry out of its modest retirement, placed it in the limelight, and advertised it as nothing else could have done. The number of students in chemistry, always a rapidly growing factor, has this year taken a great leap forward. The University of Illinois is fortunate in having completed a building for chemistry so carefully planned and so magnificently equipped. It is fortunate also in the splendid spirit which has characterized its work in chemistry, and in the remarkable number of investigations of the highest order which have been, and are being carried on in its laboratory. The state of Illinois is to be most heartily congratulated both on the performance of its university, along chemical lines, in the past and, with the space and the facilities which the new laboratory offers, upon its promise of even greater things in the future.

Alexander Smith

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RESEARCH AS A NATIONAL DUTY

The object of this paper is to emphasize the importance of material research and to lay stress on its necessity to any people who are ever to become a leading nation or a world power.

I have called it material research because I wanted to exclude immaterial research. I class under this head pure thought as distinct from thought mixed with matter. It is worth while making this distinction because, from the youngest to the oldest chemist, it is not always recognized. It is very natural for us to think we can think new things into being. Chemistry has advanced only in proportion to the handling of chemical substances by some one. When the study of our science was largely mental speculation, and the products and reagents largely immaterial,

like fire and phlogiston, we advanced but slowly. Ages of immaterial research for the philosopher's stone only led to disappointment. Successful results in modern times came from following nature, learning by asking and experimenting, reasoning just enough from one stage of acquired knowledge to ask the next question of materials.

Professor Trowbridge, of Harvard, once said:

> Before Galvani's time men were lost in philosophical speculations in regard to subtile fluids; after his experiments their thoughts were directed to the conditions of matter immediately about them. Benjamin Franklin brought electricity down to earth from the clouds, while Galvini's experiments brought men's minds down from the heights where they were lost, having no tangible transformations to study.

I will go directly to my point. We are being shown by systems of national development how important is the study of the properties of matter. There is no need to raise the questions of the war, nor of the relative originality of different races, nor to compare the gifts to scientific knowledge of the various world powers. We will go only so far as to point out that in national processes there may be a certain peculiar and useful attitude towards exact knowledge. I mean by peculiar that, as is the case of Germany, it differs in direction and intensity from that of other countries. In so far as it is useful, I want to recommend it. We all condemn it where it is abused. I want to convince you, if I can, that in the uses of science we ourselves have much to learn, and in the matter of research we are still children.

In speaking of research, I do not mean to confine my thoughts to the chemists and their knowledge and literature, but rather to that science which is back of chemistry. We may call it natural science, if we are careful. It includes, for my present purposes, all philosophy based on measurable facts. Psychology and therapeutics come under this head; so do electricity and medicine, anatomy and physics, chemistry and biology. These are inquisitive sciences, where the answers come from asking questions of nature. If I can leave with you even a faint impression of the importance of new knowledge, the strength to be gained from its acquirement, and the pleasure in the process itself, I shall feel repaid.

Research is sometimes looked upon as a remote, postponable, and especially exacting undertaking, well suited for martyrs of science and unreasoning optimists, and not at all for teachers. The historical methods of teaching have still lingering in them some of these signs. Even in our day it is sometimes said that a teacher should not be an investigator. It will take a long time to completely efface that idea, but it will be as surely forgotten as the fact that most of our older colleges were once religious centers. It is important to realize that the need, facilities and possibilities of research are all about us, retarded only by the inertia that is in us. So much useful pioneer work in all fields has been done with simple material equipment coupled with good mental equipment, that it almost seems as though this was the rule. The telegraph and telephone started with a few little pieces of wire wound by hand with paper insulation. The basic work on heredity was carried out by an Austrian monk with a few garden peas. The steam-engine came from the kitchen fire, and wireless from the tricks of a little spark gap. There was, however, the same general kind of mind behind each one of these discoveries, the mind of the trained inquirer.

Exactly the opposite belief is also quite

common—that great advances are made by sudden flashes of thought through the mind of some lucky and presumably unoccupied individual. If this were so, there would be little need for the high degree of training which is necessary for almost any scientific service in our day. We may find a simple illustration of this point in organic chemistry. We know that the artificial production of important chemical compounds, such as indigo and rubber, has been accomplished. But how many of us even begin to realize the training that was necessary and the research that had to be done before success could be claimed. The Badische Company spent seventeen years completing the indigo work after the first synthesis, and expended about five million dollars before a pound was put on the market. I might say that without at least fifty years of work by thousands of research chemists, neither problem could have been solved.

I would also be right in saying that if you removed from that structure even a part of the purely theoretical work, such as that where organic chemists spent their lifetimes testing the compounds for the imaginary double bonds of the hypothetical benzol ring, such synthesis would not have been brought about.

In my study I have a photograph of about thirty young research men grouped about Wöhler. This is the chemist of Göttingen who first discovered that an organic compound could be produced in a laboratory. It was he who also made the first metallic aluminum. The picture was taken in 1856, about as early as decent photographs were possible. Every year since 1856 that Göttingen laboratory, among others, has been training chemists in research. They have gone into fields of infinite chemical variety. Each man has been a center in some distant place, and around this center there has often been built up in turn some kind of chemical structure. Many became teachers, and their students in turn became experimenters and teachers. Many followed industrial chemistry and extended the field of the ever-increasing army of chemists. In my particular photograph is one man who in 1866 became the Professor Goessman of the Rensselaer Polytechnic Institute and was later professor at Amherst and very prominent for years in the Massachusetts State Board of Agriculture.

Since 1856 the same seeking for knowledge by renewed groups of such men has been continually going on in many foreign laboratories, but is only slowly being taken up in our country. Is it not time that we awakened to the fact that, as research chemists, we are still in our infancy? If we are ever to be a leading country in industrial chemistry, research is absolutely necessary. If such research is done elsewhere, then the major part of the advantage will lie elsewhere also.

This is one of the most difficult points for an American to recognize. Forests may be leveled by a brawny arm with an ax, canals may be dug with a dredge, but practical science needs knowledge and training, and always more training.

Scientific research, or research in the natural sciences and in the industries, might be defined as the pioneer work of the developed country. In this light it is easy to see that our turn has come. Not long ago our pioneer work was of another kind. It was opening up the undeveloped land. It was actively and well done. But the work must change because our requirements have altered.

Carl Helffereich, director of the Deutsche Bank and now secretary of the treasury of Germany, writing before the war, said:

All economic labor aims at making external nature contribute to the needs of man. It is as true of the primitive gathering of roots and berries as of the production of cyanamid or calcium nitrate. The enormous progress of modern economic technique is due to the splendid development of the natural sciences and the systematic application of scientific knowledge to economic labor. Physics, chemistry and electricity have outvied each other in their influence upon economic technique.

Speaking of the scientists, he says:

Our hermit poets and thinkers converted themselves more and more during the past century into practical creative workers, and an enormous expansion of activity has resulted from the progress of the pure and applied natural sciences.

American chemists have had German chemists pointed to as examples almost long enough, but there is some value in concrete examples, and I can not refrain from comparing our own impoverished condition in the matter of nitrogen to that of Germany.

Excepting one or two minor attempts, we Americans have made almost no study of the fixation of atmospheric nitrogen. I want you to realize the varied and expensive researches, mostly carried on abroad, which were required to reach the present position of the nitrogen question. There were in Germany and, by German capital, in Scandinavia, several direct oxidation processes, carried through the experimental to the practical commercial stage. The Schoenherr process is one of these, the Birkeland and Eyde process another. The direct combination of nitrogen and hydrogen to form ammonia has been successfully developed in the German Haber process, and the cyanamid process, with all its products from carbide to ammonium nitrate, was developed in Germany. There they used not only the peculiar reactions of calcium carbide with nitrogen, but the production of the nitrogen from liquid air, the reaction between water and cyanamid to form ammonia, and then an oxidation process for obtaining the nitric acid. The oxidation of ammonia to nitric acid by such methods as the Ostwald process has been studied by many investigators since 1830, and several different schemes are now in use abroad.

At the time most of this research work was under way it was not at all clear what use was to be made of it. Much of it was purely academic research, but it was clear that without the knowledge itself certainly no use at all would be made of it.

I do not want you to look at research as an old, established utility. I want you to see it as I do: a powerful factor proved in the advance of the industrial welfare of the foremost countries, and a world-experiment of less than a century's trial, but something still unappreciated in America. It is true that the earliest man and many of the lower animals accomplished ends by research, but I refer now to research in the natural sciences and to the research which in our day is necessary to our desired activities. These sciences are already very highly developed, and an equally advanced education is demanded. For example, if I wish to cure physical ills, I can not expect to do it by reciting ancient incantations, nor by using roots and herbs, as was once customary. I must first familiarize myself with an accumulation of previous experience. I must study anatomy, physiology, chemistry, bacteriology, etc. This is a relatively recent world-condition. Conditions are similar in all the applied sciences. The accumulated knowledge in any field is already very considerable, and to get on to the firing-line of useful work one must go up past the baggage-train of knowledge and experience. There is something in the blood which makes an American naturally hate preliminaries. It will be a great day when we see how important

preliminaries are. The hospital surgeon well knows how much more willing the young interne is to actually handle cases, if it is only to administer the ether or the iodine, which any nurse can do, than he is to study the theory of ether as an anesthetic, or of iodine as an antiseptic, which perhaps no nurse could understand. The young student of mechanics thinks he could have devised the steam turbine if it had not been done before his day, but when he comes to study the problem as it has actually been developed, he finds the same old kinetic theories, differentials and integrations which he spurned as too theoretical when he sought a short road to engineering.

I want you to realize that in America we are going ahead in future at a rate dependent entirely upon our preparation. Laboratories are a relatively modern thing. In most of the sciences they are a development within the lives of men now living. I want you to see that we must be foremost in systematic, organized research, or we will be distanced by other countries which already well recognize the value of new knowledge.

When so much of our material welfare, the condition and extent of our manufactures, the quality of our agricultural efforts, and the health of our people, depend upon the rate of our acquirement of new knowledge, there ought to be much greater effort made along the lines of research than is at present the case.

We call knowledge power, but we need to see that new knowledge is like a second power to power.

I'd rather be a little Moses than a big Jeremiah. I'd rather point a way to a promised land, however remote, than talk about our lamentable conditions. But we Americans are not entirely imbued with the spirit of active and efficient service. We are a preliminary experiment on the possibility of operating a competitive nation in a democratic manner, but we don't care much about it. We have about as little interest in the wonder and elasticity of nature, the laws of materials (except where they affect our stomachs and our health) as had Darwin's starving Patagonians. With us the spirit of the hive is confined to the bees. Germans and Japs make better scholars than we do, and a Chinese laundryman sticks longer to his daily job and talks less about it. We are living in the Garden of the Gods, but we are still eating grass.

Is there no significance in the fact that many of our colleges are better known through their foot work than their head work? Is it not significant that the Y. M. C. A.'s dotting our land are as strong in bowling-alleys as in education, and that most of our religious training goes to the heathen? Is it a sign of health that so large a portion of our newspapers are paid to feed us with results of useless experiments between prize-fighters? I think the stadium should be the accessory of the laboratory, not the temple of the oracle; and that in reality a research laboratory is more compatible with the object of a university than is the more common training-table. I do not mean to be too insistent as a critic or too pressing as an advocate, but I hate to see my own country such a trailer as it now is. I hope the conditions are changing, but I know they are not changing fast enough.

All service is based on knowledge, and knowledge is an ever augmenting thing which almost any one may increase. If the stock is *eternally* useful as it is, how great must be the value of the indestructible increments which any one may produce. I do not think due reverence is given to new knowledge. I want to illustrate.

Some time, somewhere, centuries ago, the slag of a fireside appeared transparent,

some one tried to learn more about it, and so, ultimately, glass was made. Research is still under way on that very material, and countless numbers of men have added to the knowledge. Glass has kept the cold from the house. It has let in the light. It has renewed our eyes as they have worn out. Through telescope and microscope it has shown us the greatest and the smallest things of the universe. It has bottled our drinks and held our lights. Every year still adds new service, just in proportion as experiments add new knowledge of glass. To-day we hear of new glass permeable to ultra-violet light, glass opaque to X-rays, and glass for cooking utensils. Not one of these little increments will ever be lost, but will continue in use, so how highly should we value them? Why did we delay so long in coming thus far, and how far or fast may we still go?

Research is preparation. It is preparing in our decade for the problems and the necessary work of the next. There are various kind of preparedness. We are hearing a great deal about one of them nowadays—immediate preparedness for national defense. But there is a more far-sighted preparedness that no one has adequately described and of which the building of new laboratories is a sign. This type is the very best kind of preparedness for national defense, if begun in time. The continued study of the secrets of nature, the uncovering of buried treasures which always seem buried just deeply enough to develop the digger—these are the criteria of a strengthening nation.

Research presents a way, and the only certain one, of insuring peace, of preparing successfully for defense, and of being successful in war. It is the lasting, undeviating factor which has always dominated. This may sound bold and entirely inconsistent in itself. It is all true. Can we learn to see it? From the military expert to the anthropologist, thinking men recognize that for over 100,000 years war has been almost continuous on the earth. The inventors of chipped flint successfully fought those inferiors who had not experimented with flint. There were then no better arms. These also got their game even when it was scarce and other means failed, and so they continued to survive. This little and early example of survival was repeated a great many times before our present complex world conditions were reached, and will as surely continue to be repeated. The fundamentals were always the same. A 42 cm. gun is only a better flint. Trinitrotoluol is only a more modern sling. Arms and ammunition have changed, but just so have also changed the myriads of other important accessories to survival. This is the important point. Good guns go with good clothes, and niter is used both in fertilizers and in guncotton. The signs that we are improving in our civilization will also indicate that we are growing in our powers of national defense, but this should come rather as a consequence than as an object. And we Americans must not stand still. The world has always been improving, and the real growth and development has come to those nations which have been responsible for the original research work and not for the mere storage or conservation of the knowledge.

The first or fundamental discovery in any series is not the only important one, so I am going to take an extreme view and say it is only the continuation of research which is of any considerable importance to us. The fundamental discoveries may be like seeds, but the values are like growing plants. An acorn may correspond to the work of a Henry or a Faraday, but the great and growing tree of electrical or chemical work corresponds more nearly to

the living state of the oak species. We are much more interested in what is to come than in what has already been accomplished.

I realize that I ought to illustrate this appeal for research by concrete examples of things to discover. I know the feeling of the chemist who is mentally compressed by the mass of investigation work which has already been done and by the known facts which seem already to entirely cover all possibilities; but I know, too, that the future will make use of knowledge for which we now have no vocabulary and no powers for comprehension, and so could not possibly anticipate. If, then, I try to illustrate the search for new knowledge, you may be sure my illustrations will be inadequate.

In the first place, I can not be reckless enough. This I learn from looking backward. I would not have dared suggest that a dozen good men should study the little hydrogen generator of the freshman laboratory, to see what was in it. If I had, I suppose I should have suggested a research on pipe organs, because of the singing hydrogen flame, or on bombs, because of its explosion. But some one tried synthetic ammonia, others Zeppelins, and others the cutting and welding of iron. When I see in our factory the three score men now using oxhydrogen all day for this latter use, I am impressed with the eternal proximity of new and useful knowledge. A very few years ago, two or three times as many men would have been necessary to do this work in the old more difficult and less satisfactory manner.

The most natural suggestions for research are those simple ones referring to chemical elements. There are still plenty of unknowns among the elements, and of one thing we may be sure, there are certainly no two alike. Any chemist who wants to add to chemical knowledge need not go beyond the list of elements for his subject. The properties he discloses will every one of them be sometime a help to his science and of service to his country. As far as possible, his country will reward him with patents if he asks them.

We ought to begin at the points where others left off, and continue the research of the chemical elements. One reason why this appeals to me is that I have seen so many recent applications of entirely new knowledge of elements in my own work. I will just mention tungsten, molybdenum, boron, argon, silicon, magnesium, titanium, thallium, vanadium and chromium, which, because of properties not known until recently, are nevertheless already doing commercial service in our restricted electrical field. Surely we know still far too little about these elements, but we know less about some others.

If now the chemist, still forgetting the compounds and narrowed in his researches to the elements, and then perhaps to the metals, and finally to a single element, still asks, what shall I do? I would refer him to the isotopes of his element. Our American Richards, supporting the researches resulting from the studies of radioactivity, has shown that there are two leads. They are somewhat different, but can not be separated easily. Of course some one ought to separate all isotopes, and then there is plenty of room for research on the single isotope.

One of the great needs of the country which reflects on us chemists and calls for immediate research is that for American potash. There is no supply in sight which is nearly comparable with the German deposits, and our fertilizer and other industries will certainly suffer because of this deficiency. We have plenty of feldspar calling for a simple process for removing

the potash it contains. We have oceans of sea water carrying plenty of potash, if we knew how to extract it. Don't say it can't be done, for it is already done by miles of seaweed. Why should we confine ourselves to trying to take it away from the seaweed, instead of learning what the seaweed knows about getting it from the water? You will look supercilious, but until a large number of chemists have studied semipermeable membranes, there will always be this lack of understanding of those simple reactions of living matter going on around us. There will always seem to me a possibility of doing such physical and chemical processes more nearly as we may wish to do them when we know how these operate.

When nothing new is being done by us it will be a sure token of our decay. When we stop increasing our experimental activities or fall for a considerable time behind the activities of other countries, we may expect to see our light become merely a memory, like that of Greece or Rome. Thus far we Americans have not reached a fair average as investigators in natural sciences, and yet we have incomparably superior conditions for the growth of research. I can not look beyond the period when research shall cease in a country and still imagine that country a power in the world.

There are no sharp lines to be drawn through research to separate pure from applied, scientific from practical, useful from useless. If one attempts to divide past research in such a manner, he finds that time entirely rubs out his lines of demarcation. At this particular time, however, one may imagine a more or less zigzag zone which serves to divide research in a commonly accepted way.

In a manufactory the price of a new product should include the cost of research. No matter how complicated the system, this is always true. Otherwise the industry would ultimately commit suicide. In practise it is common to apportion to particular products the cost of their separate development, and to fix the price so that within a reasonable time, or by a reasonable volume of sales, the so-called development cost may be wiped out. Thereafter the product may be sold on the basis of the continuing cost of actual production. While this system is extensive, it does not cover the cost of many of those original researches which may have been absolutely necessary. The argon tungsten lamp, in its development cost, did not carry the expenses of Rayleigh and Ramsay's work, and so there will probably always be some such classification of research work necessary.

Under such a classification, the part of research I am most interested in promoting is what we may call the unpaid kind, not because it is cheapest, but because it is the most valuable. It is most neglected, most poorly understood, most in need of appreciative support in America.

The separate industries do not need encouragement in research nearly so much as the nation needs it. The industries can be depended on to estimate its value to them, for they take annual inventories. But a country which keeps no books seems to have to depend on instinct and environment for its most valuable research work.

It seems to me that our American colleges have been shortsighted in this respect. This may be explained by the rapidly increasing demand in our growing industries for analytical chemists and chemical engineers, who could at once meet the existing industrial requirements. This demand has kept the chemical departments of our colleges and technical schools very busy with the elementary and analytical side of chemistry and left little room for the synthetical or experimental side. It has also naturally tended toward the development of highly

efficient organizations, equipments and corps of instructors for the preparation of the one type of chemist, but this very success seems frequently to make impracticable the training of men for research. The conscientious American professor has usually devoted his life to bringing his students up to a certain promising stage of interest in science and experiment, only to see them scatter before they have had any experience with questioning nature, or have tried any unbeaten chemical byway.

While I am greatly interested in what might be done for science by technical research laboratories in the industries, I am sure that the university must be the important factor in guiding the pioneer work if we are to be a sufficiently advancing nation.

Let me recall recent words of President Wilson:

> I know I reflect your feeling and the feeling of all our citizens when I say the only thing I am afraid of is not being ready to perform our duty. I am afraid of the danger of shame. I am afraid of the danger of inadequacy. I am afraid of the danger of not being able to express the correct character of the country with tremendous might and effectiveness whenever we are called upon to act in the field of the world's affairs.

These words ring true. The American spirit is characterized by them. But think further a moment. They refer to a fear based upon an entirely corrigible defect. The cure is in our hands. The time when we are called upon to act in the field of the world's affairs is *now*; but it was yesterday, and it will be to-morrow. I maintain that no nation can effectively act in that field at odd or selected moments. It is either doing it much of the time, or it is likely to be unable to do it any of the time.

WILLIS R. WHITNEY

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THE COMMITTEE ON POLICY OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE Committee on the Policy of the American Association for the Advancement of Science met on April 17, 1916, in Washington. Messrs. E. L. Nichols, *chairman;* Charles R. Van Hise, *president;* R. S. Woodward, *treasurer,* J. McK. Cattell, W. J. Humphreys, A. A. Noyes, Stewart Paton, E. C. Pickering and L. O. Howard, *permanent secretary,* were present.

The committee on delegates to the meetings was instructed to make an especial effort to secure delegates from the educational and other scientific institutions to the New York meeting, as this will be the first of the large four-year meetings.

The treasurer and the permanent secretary presented financial reports which were ordered printed in SCIENCE.

The committee on new affiliated societies reported that the following societies had been admitted to affiliation: American Genetic Association, Eugenics Research Association, Illuminating Engineering Society, Wilson Ornithological Club, and the Mid-West Forestry Association. The American Institute of Chemical Engineers and the American Society of Heating and Ventilating Engineers were invited to become affiliated.

The treasurer reported with regard to the Colburn bequest and stated that approximately seventy-eight thousand dollars ($78,000) in cash and bonds had been turned over to him by the executors. On motion, the treasurer was authorized to convert cash to the amount of eighty thousand dollars ($80,000) into securities approved by the state laws of New York and Massachusetts for savings banks and trust funds. On motion, it was directed that these investments be made with the advice of a committee of three, of which the treasurer and Mr. A. S. Frissell shall be members, they to select the third member.

The permanent secretary announced the death of Professor Thomas J. Burrill, the chairman of Section G, stating that he had sent, in the name of the committee, a tele-